pliquée doit finir par épuiser les forces d'un seul homme, surtout comme je suis obligé de vouer la plus grande partie de mon temps à une activité différente. Il est également clair, qu'une entreprise, n'ayant pour appui que l'initiative individuelle, ne peut être poursuivie avec autant d'exactitude et de perfection, qu'étant basée sur une institution publique. Il me semble, enfin, que ces recherches sont d'une telle importance, qu'il est à désirer, qu'elles soient continuées si énergiquement et sur une échelle si complète que possible. Cela ne peut se faire, selon mon avis, que par la création, par l'état, d'un établissement spécial pour ces recherches. Les principes sur lesquels je voudrais voir basé un tel institut, les voici :

(*a*) L'institut sera établi à Drontheim (63° 27′ lat. N., 8° 5′ long. E.P.), endroit à peu près central dans le pays et mis en vive communication avec l'étranger par des bateaux-à-vapeur et des chemin-de-fer. Situé à 30° S. seulement du cercle polaire, il est assez rapproché de la zone le plus active des forces du magnétisme terrestre, ce qui, entre autres choses, est aussi prouvé par le fait que, pendant l'hiver de 1880–81, non moins de 62 aurores boréales ont été observées par un de mes observateurs, et que les perturbations télégraphiques y sont aussi très-fréquentes ; dans la période 1 Julliet 1881–30, Avril 1881 on en a compté a pas moins de 122 jours (plus de 40$_0$/°) dont une partie insignifiante provenant des orages.

(*b*) Il sera construit pour l'institut, sur un emplacement à l'horizon libre, un édifice spécial, situé de manière à ne pas être gêné par la lumière artificielle. Sans compter l'appartement du directeur, l'édifice doit contenir une salle de travail, des cabinets pour les instruments, une tour, contenant une chambre à vue libre dans tous les sens et une plateforme.

(*c*) L'institut tâchera de devenir le point central des observations des aurores boréales en Europe. Tout en faisant lui-même toutes les observations possibles, il veillera sur la régularité des observations dans toute l'Europe septentrionale, dans l'Islande et dans le Groenland, il en chargera les navires navigant les mers arctiques etc.

(*d*) L'institut se fera également le centre des observations des perturbations télégraphiques, recues de toutes les lignes télégraphiques de Norvége et de Suède. Le rhéographe cité plus-haut sera employé à des observations nocturnes sur une ligne longue. De semblables recherches seront faites à l'aide des lignes téléphoniques de Drontheim.

(*e*) Sans compter les spectroscopes, les théodolites pour les aurores etc., l'institut sera pourvu d'une série d'instruments de variation, à l'action spontanée, placés dans un pavillon particulier.

(*f*) L'institut publie tous les ans les observations recueillies et classifiées par lui, ainsi que leurs résultats.

Je suis persuadé qu'un observatoire, basé sur ces principes, pourra rendre d'importants services à la science, et j'espère que la réalisation de cette idée sera accueillie avec joie par le monde scientifique.

J'adresse cette communication à vous, M. l'Editeur, et à d'autres autorités reconnues sur ce terrain de la science, pour que cette question soit soumise à votre jugement. Je vous prie de vouloir bien me communiquer votre opinion sur les mérites d'un tel institut, afin que le poids de votre nom me serve d'appui dans mes efforts pour la réalisation de mon projet—si toutefois l'idée gagne votre approbation, ce dont je ne doute pas. Si vous y ajoutiez encore la bonté de formuler des propositions eventuelles et des conseils pour l'organisation de l'institut, pour son plan de travail etc., je vous serais également très reconnaissant.

Dans l'espoir d'être honoré d'une prompte réponse, qui pourra être conçue dans quelque langue que vous deviez désirer, j'ai l'honneur, M. l'Editeur de vous prier d'agréer l'expression de mes sentiments les plus distingués.

Bergen en Norvége, Juin SOPHUS TROMHOLT

Hydrophobia and Snake-bite

IT has been the fashion of late, in newspaper and other commentaries on the numerous experiments in search of an antidote for serpent-venom, which are now attracting general notice in different parts of the world, to compare snake-bites with hydrophobia, and to raise hypotheses and suggestions that the remedy, when it is discovered, for the one, will be found also to effect the cure of the other. Such a surmise, one would imagine, must be based on a very superficial knowledge, even of the rough outlines of the two affections, and in total ignorance of their pathological characteristics. What are their points of similarity and distinction, dismissing all rhodomontade involving such terms as "ferments" and "active principles," "germs," and "vibriones"—expressions which are very convenient to make use of as signifying an *unknown something*, but which, in their literal meaning, indicate definite actualities, the existence of which has certainly never been shown by tangible demonstration in the present instances? Hydrophobia and blood-poisoning by a venomous snake-bite are alike, then, in being produced by an animal poison, commonly conveyed by the living animal ; in being of intense virulence, accompanied by many parallel symptoms denoting violent constitutional disturbance, and great prostration of the vital powers ; and, *in some cases*, in leaving certain post-mortem lesions which are identical. But there all likeness ceases ; and even without considering the grand differences which exist between them, however viewed, I think we shall find these items of resemblance when analysed a very insufficient foundation for the theory of the probability of a common antidote. To begin with, the poison in one is a natural secretion, provided for the distinct physiological purpose of enabling the reptile to secure its prey ; in the other, a new and morbid product generated by disease in a secretion naturally innoxious—whether by chemical decomposition or by the formation of new compounds from materials pre-existent there or from others specially eliminated from the blood, we cannot say. For it is to be observed that neither the microscope nor chemical examination has afforded us any clue to the mysterious ingredient which constitutes the toxæmic property of these fluids as yet. Then, in the second place, as to the acuteness of the symptoms of vital disturbance ; neither the symptoms themselves nor their severity are specific to these affections, but are such as might be produced by any grave interference with the relation of tissues or function—a deadly mineral or vegetable poison, the rapid course of a zymotic disorder, violent concussion, or the rupture of an internal organ. Lastly, the occasional identity of the appearances after death from snake-bite with those which are constant in hydrophobia (notably those observed in the spinal cord) are little to be relied upon, partly on account of their comparative infrequency, and partly because they are not in themselves primary evidences of the introduction of the poison, but *tertiary* to it—secondary to the effects which result from the action of the altered blood and its new and vital function on other tissues. One is compelled to attribute such phenomena to something more than passive contamination (as the idea giving rise to some recent experiments would seem to be), and to recognise a defined potential agency in the new formation.

The fact that the hydrophobic poison is located, and undergoes a period of incubation at the seat of the original wound, even long after cicatrisation, and is not diffused in what we vaguely call the "system," draws a broad line of separation between the pathology of this disease and snake-poisoning, and renders it perfectly unique in the category of ills known to medical science. The constitutional symptoms never appear in less than three weeks after the bite, rarely under six, and may be delayed for twelve months. Could there be a greater contrast to the *impetuosity* of the changes sequent on inoculation with the poison of a serpent? Both the latter fluid and the saliva of a mad dog are incapable of producing their characteristic effects when absorbed through mucous membrane, but that is a circumstance common to most animal viruses.

There is a far greater similarity between the course of some of the rapidly fatal tropical fevers and a snake-bite, than there is between that injury and hydrophobia. The inception of a morbific agent—though of what nature, or through what channel, is uncertain ; the almost *instant* development of acute symptoms—for, if there is any period of latency, it must be a very brief one ; the intense disorganisation of the economy produced within a few hours ; the nearly inevitable termination in death—all suggest a comparison which will not be thought strained or far-fetched by those who have witnessed the progress of these appalling diseases. My friend Dr. Fairbairn, of Rio de Janeiro, who has probably seen more of yellow fever than any man alive, and who has certainly met with greater success in his treatment of it than any other physician whose experience has been placed on record, called my attention to this resemblance, with which he had long been impressed, living in a region which afforded plenty of opportunities for observing both ; so much so, that he expressed a conviction that many points of identity of process and morbid anatomy would eventually be revealed. His last words to me at parting were : "Now, mind—if you ever

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discover the antidote for snake-bites, you'll have a cure for yellow jack!" Arthur Stradling

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The Rainfall of the Globe

In reference to a paragraph in an article on the rainfall of the globe in Nature, vol. xxvi. p. 206, Prof. Loomis states that the heaviest rainfall is met with in the rain-belt, which surrounds nearly the whole globe lying between the north-east and south-east trade-winds.

Having been engaged in collecting records of rainfall at sea for some time back, I may take the opportunity of saying that I have received data enough to enable me to give an estimation of rates per annum for this rain-belt.

That for the Atlantic Ocean is calculated at 133·37 inches per annum, that for the Indian Ocean at 80·55 inches per annum, that for the Austro-Chinese Seas at 107·96 inches; but none has yet been made out for the Pacific Ocean, owing to absence of observation altogether from that quarter.

In physical atlases the rain-belt is continued across this ocean in the same latitudes as it is found to exist in the Atlantic and Indian Oceans, but as yet it is only conjectural, and it may probably be found to cross in other spaces by direct observations taken at sea. The observance of rainfall on islands in the open oceans would appear to afford but imperfect means of judging of the rainfall at sea surrounding them. It is frequently found that they differ very materially, as at St. Helena, the island may be covered with mists, invisible, while the ship outside is sailing under a clear sky and fresh breeze.

The ocean rainfall, therefore, can only be made out by observations on board ships, and these are not easy to get, and also take up a long time in effecting. W. J. Black

Caledonian United Service Club, Edinburgh

The Recent Unseasonable Weather

Mr. Archibald's letter on this subject displays great ability, and is deserving of much attention by meteorologists. Nothing can be, I think, more interesting and important than a proper interpretation of the meaning of the facts of the weather of the late extraordinary and contrasted seasons. Will you, therefore, allow me shortly to make a few remarks regarding the comparison pointed out by Mr. Archibald as existing between the weather of India and that of North Europe.

In the "Report on the Meteorology of India," 1877, the reporter, referring to the Himalayan regions, says: "There are two periods of cold, (1) when the snow is accumulating; and (2) when it is dissolving; and the first occurs in January and February, the latter in April and May." Again, he says: "The conclusion appears to be very strong that during the early months of the year, one very important factor in determining the peculiar features of the season is, the amount of snowfall and of snow accumulation in the Himalayan regions during the winter."

For six years past I have observed the same thing hold *generally* in the north of Europe, a cold winter being followed by a late spring and an ungenial summer, and *vice versa*. This I would therefore be inclined to regard as a general law. The weather of June, however, serves to indicate the difference between the meteorological conditions of India and North Europe. It has been ungenial, I think, solely, on account of the exceptional force and warmth of the winds of winter causing a vast detachment of the ice in the polar regions. This ice has floated into lower latitudes—has come much nearer to us—and has produced, in melting, icy winds. These commingling with the warmer tropical winds, have produced in their turn the recent changeable weather.

Our cold spring winds usually come from the east of north, but the prolongation of cold winds which we experienced in the middle of June, came from the west of north, indicating their origin to be in the masses of ice floating for the most part probably between Iceland and the American coast. Thus the movable ice has caused a high atmospheric pressure and a low temperature. The fixed ice, however, which forms by far the largest area within the Arctic Circle, has been during this winter relatively diminished, and from it, therefore, we should expect less incursion of cold winds; therefore a finer summer.

While, then, the chief influence of warm westerly winds in winter is, as I believe, to produce a fine summer, their minor influence must be, particularly when strongly developed, as they have been during the past winter, by detaching an unusual quantity of Arctic ice, to produce unseasonable weather in early summer.

The same exception would take place in India, if we could suppose some part of the winter's accumulation on the Himalayas to be carried at the close of a severe winter down into the northern plains so as to distribute in melting, volumes of cold air throughout the otherwise warm atmosphere.

Dundee, July 3 David Cunningham

Is the Axis of a Cyclone Vertical?

I am not aware if it has ever been suggested, in explanation of the frequent (or rather, usual) incompleteness of cyclonic disturbances, that the axis of the cyclone may be inclined, and consequently only one side of the disturbance affect the earth's surface, the other half being at a greater or less elevation, according to the amount of the inclination, and thus (so far as wind currents are concerned at least) lost to us.

My own observations of storms in this country point to a southerly inclination of the cyclonic axis. I should be glad to know if observers in the southern hemisphere have traced any indications of a *northern* inclination in the cyclones there.

J. A. Westwood Oliver

Belle Vue, Springburn, Glasgow, June 25

The idea propounded by Mr. Oliver, that the axes of cyclones are inclined, is no new one, nor is it the first time that a *southerly* inclination has been inferred to exist, to account for the preponderance of winds belonging to the southerly quadrants, and the comparative absence of those belonging to the northerly quadrants of cyclones in our latitudes.

The value of Mr. Oliver's opinion on this point must depend to a great extent on the nature of the observations on which he relies.

This supposed southerly inclination was formerly attributed by Andrau and other Dutch writers (according to Réclus), to the fact that a cyclone, starting from some point near the equator, must have its rotation-axis initially inclined to the terrestrial axis nearly at right angles, and that as it moves from thence polewards, the direction of its rotation-axis remaining fixed in space, it must *apparently* become gradually more and more inclined to the local horizon in a southerly direction. This explanation is ingenious, but there are many considerations, both theoretical and practical, which militate powerfully against it.

Another view—that of the Rev. W. Clement Ley, derived from observation (principally of the upper clouds)—makes the axis of a cyclone incline backwards as regards its direction of translation, and in favour of this notion, the retardation in the occurrence of the barometric maxima and minima on the summits of Mount Washington, Pike's Peak, and Mount Michell, noticed by Prof. Loomis, has been cited.

Ferrel, however, remarks that "a retardation of just about the same amount is observable in the occurrence of the times of maxima and minima in the diurnal changes of the barometric pressure at the summits of these same mountains, which cannot be explained by means of cyclones with reclining axes," so that in all probability the same cause acts in both cases, and is independent of any such special quality of cyclones as that inferred by Mr. Ley.

The hypothesis of Mr. Ley is, moreover, so much at variance with mechanical principles and with what we should naturally infer would take place, that, as Mr. Ferrel says, "we must hesitate to adopt it, without seeking further for some more plausible hypothesis to explain the observations."

The theory of cyclones, as developed by Ferrel and others, makes it far more probable that if there is any inclination at all, it will be *small*, and *forwards*, not backwards.

Ferrel thinks it possible that the elliptical form of the isobars and rain-areas is partly due to this forward inclination of the axis.

Moreover, the preponderance of southerly and westerly winds in our cyclones cannot correctly be adduced as an argument in favour of the southerly inclination of their axes, since it is mainly due to the fact that our cyclones are for the most part secondaries, moving within the periphery of a large, nearly permanent cyclone, whose centre generally lies not far from Iceland, and thus those winds and gradients predominate, which would tend to occur in that part of the large cyclone where we happen to be situated. E. Douglas Archibald

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